Dear editor,

we have gratefully received the reviews and tried to address all concerns and recommendations appropriately. We identified the main point of concern that we have claimed the persistence of the findings in turbulent conditions. We agree that our claims were too strong to be justified without further support of data and we are grateful that both reviewers clearly mentioned this. We addressed this by adding the results for turbulent inflow conditions, which unfortunately caused a delay in the preparation of the revision.

Best regards,
Christian W. Schulz
on behalf of the authors

Dear reviewers,

thank you very much for your carefully conducted reviews and the significant amount of time, you have invested to help us improving the manuscript. We believe that the quality of the manuscript has been significantly improved and hope that you may have the same impression.

Please find our detailed responses to your comments below.

Best regards,
Christian W. Schulz
on behalf of the authors

Reviewer No. 1

The brief communication: "A novel wake mixing phenomenon and key parameters for wake recovery of floating wind turbines subjected to surge motions" by Schulz et al., describes the impact of motion conditions and turbine operating conditions on the wake deficit and coherent structures in the wake of a surging two-bladed wind turbine. The work is well structured and presented and definitely represents a valuable contribution to Wind Energy science, especially due to its relevant contribution in extending the current understanding of wake dynamics of floating wind turbines under imposed surge motion. After reviewing the manuscript, I believe that it can be accepted for publication after addressing some concerns from my side. In particular, while most of the comments only require minor editorial changes and further clarification (as explained in detail below), there is a more general comment which is relevant to the whole study and which should be addressed before publication.

General comment

If I am not mistaken, the authors are only showing in this manuscript results under laminar inflow conditions. From some sentences within the manuscript, it is hinted that the authors also performed some tests at higher turbulence levels, even though this is not explicitly stated (and again, no results in turbulent conditions are shown). Nevertheless, the authors make, I believe, some significant statements concerning the extension of the current results to turbulent inflow conditions. Given the current body of literature on floating wind turbine wakes, which has started to show how turbulence may have a significant impact on wake dynamics of floating wind turbines under imposed motion, it is difficult to accept such extension of the results without any data to support it. Additionally, I found some statements "too strong" or too concise, for example in Sect.2, where it is only stated that results can be generalized to turbulent conditions. Additionally, I would say that this statement about the extension of the results to turbulent inflows, contradicts the brief sentence in the conclusion, where it is stated that the impact of motion reduces with inflow turbulence and may disappear for turbulence levels above 5%. In

conclusions, I believe that some evidence or reference about results under turbulent inflow could be provided(for example, was the same trend in wake deficit under different Strouhal numbers observed by the authors in some preliminary tests under turbulent inflow?) or this discussion should be further expanded upon, highlighting this possible limitation of the study.
- After some time has passed and we reviewed the manuscript ourselves again, we clearly see the point of the reviewer and are very happy to receive this feedback. We decided to include the results under turbulent inflow, which has unfortunately caused some delay of the revision. We hope the reviewers find our conclusion s appropriate in light of the added results.

Additional comments:

Line 24: The authors refer to motion-induced unsteady aerodynamics, which is a generic term probably referring to any of the 6DOF. Are the authors commenting on a generic motion (including other DOFs) or only to surge motion? In the latter case maybe this point should be clarified for the reader, as I am not sure if this outcome can be generalized. In general, the scope of this section should be clarified as it complicates also the interpretation of the rest of the section.
- Thanks for this hint. The literature review generally describes the impact of 6-DOF motions on the wake recovery, since most studies cited deal with at least two motion types. Although our work focusses on surge motions, we believe that our findings will also be of large importance for platform pitch motions. As described in the text, these can be considered as a superposition of a tower top surge motion s and rotation s, w hile we believe that the surge motion part is dominant in terms of impacting the velocity deficit. Therefore, we would like to keep the general scope of literature part. However, we specified the parts of the literature review that explicitly relate to surge motions. In addition we made more clear that our scope is limited to surge motions:

The present study focuses on surge motion-induced wake recovery. In this context, we identified three knowledge gaps the current literature, which we consider as potentially relevant for the future development of floating wind.

Line 25: The authors state that the unsteady impact on the loads can be determined as a function of the motion frequency or Strouhal number. Given the stated objective of the paper, which aims to clarify which parameters govern the aerodynamic response of floating wind turbines, I believe this is not sufficiently clear for the reader and possibly increases confusion. In fact, I wonder if the authors are underestimating the difference between the two parameters. In fact, if I am not mistaken, the aerodynamic response of the turbine could be a function of the Strouhal number but not of the motion frequency. For example, the aerodynamic response of a floating wind turbine could be the same when the frequency of motion and freestream velocity are modified so to keep the Strouhal number constant but be different if the frequency of motion is changed while keeping constant the inflow velocity. The same concept applies the other way around, the aerodynamic response of the turbine might be a function of the frequency of motion but not of the Strouhal number. The authors should clarify what they mean with this sentence and what is the governing parameter, if one has already been identified in the literature.
- We are grateful for this hint. Our aim was to keep the fist lines leading into the topic simple for readers not familiar with the mentioned dimensionless numbers. However, for the more knowledgable reader, there is a clear difference between motion frequency and Strouhal number and - as the reviewers stated - we should not mix this up. We corrected the sentence accordingly. In addition, we believe that the reviewer may have noticed in the later parts of the manuscript that we are indeed aware of the difference between the absolute frequency and the Strouhal number.

Line 26: Would this scaling not apply for example to pitch or yaw motion? In the title the authors clarify that the work concerns the analysis of surge motion, however this limitation is not clarified in the introduction.

- We clarified this.

Line 30: The list of references is definitely not representative of the full body of literature on floating wind turbines wakes. I am not sure if the authors are analysing only a subset of studies and if so, how are they selected. A small comment should be added at least clarifying that this represent only a subset of current literature.
- Since the number of references is limited in the present type of publication (Brief communication), only a small, carefully chosen number of studies could be included in this literature review. We have added a footnote to clarify this.

Line 33: In general, if I am not mistaken, the increase in wake deficit is only visible after a certain distance from the rotor and is not immediately noticeable in the near wake. The authors should clarify where this improvement in wake deficit are noticed, in the mentioned conditions.
- We agree on this and added a clarification.

Line 45: In the introduction, no discussion is carried out about the impact of the amplitude of motion. Do the authors believe that this has no effect on wake recovery, even for very small amplitudes? In general, I would expect TLP platforms to possibly be characterized by much smaller amplitudes, which could mean that the impact on the wake deficit as a function of reduced frequency might be substantially different than in the case of other more common substructures.
- We agree that the motion amplitude has a relevant impact on the wake recovery. In fact, we believe that wake recovery is governed by the combination of motion frequency and amplitude. The impact of the motion amplitude is included in \Delta V* ~ f*A, which is one of the two independent motion variables that drive the wake recovery in our opinion. We added a sentence to clarify this:
It was also found that higher motion amplitudes A_p tend to amplify this effect. However, no clear distinction between the impact of A_p and ΔV^* was made.

Line 57: I believe there is a typo and "about" can be removed.
- Thanks a lot for this hint.

Line 63: The TUHH model is characterized by a two-bladed configuration. Could this impact the outcomes of this study in comparison to a tree-bladed rotor, which represents the most common configuration?
- We agree that this should generally be discussed. From ou r point of view, the wake behaviour can be considered as a non-linear dynamic system, where the near wake flow field directly behind the rotor is the input, while the far wake field is the output. The figures shown in the paper shall help to understand and characterise how this dynamic system impacts the flow field while it travels downstream. The question whether there is a difference between a two- and a three-bladed rotor could therefore be translated to another question: Does the system input change for a two-bladed rotor in comparison to a three-bladed one? From our point of view, the main characteristic of the near wake field is the location and strength of the vortices emitted from the blades, where the tip vortices are most prominent . The vortices fo r m a helix downstream the rotor. In our case, we chose a nominal TSR of 9, which is 3/2 times higher than the TSR of a three-bladed model turbine e.g. used by Messmer (2024). Therefore, an observer in the fixed reference frame would count the same number of tip vortices per time instant passing downstream. In addition, we maintained a thrust coefficient similar to other three-bladed model turbines. As a consequence, the momentum deficit - and finally the velocity deficit - in the wake is comparable. This leads to the fact that the tip vortices are travelling downstream with a velocity comparable to other three-bladed model turbines. Finally, these two characteristics lead to a wake, that has a comparable tip vortex helix (diameter and pitch) as well as a comparable strength of the tip vortices.

However, the limitation of the manuscript length prevents us from discussing this and further relevant aspects in detail. This will be done in a follow-up publication, where a lot of more details will be discussed.

Line 63: What is the blockage of the experimental setup? Could this affect the wake response of the turbine?
- We have added the blockage ratio of 7.5% to the manuscript. However, for the same reason as stated above, we did not add a discussion on this, since we find the the topic comparatively complex. Of course, a discussion on this will be included in the follow-up publication.

Line 67: I may be mistaken, but it feels like this statement about laminar results possibly being generalized to turbulent conditions directly contradicts the introduction and the findings of some of the same authors in previous work concerning wake dynamics of floating wind turbines under imposed motion and increased inflow turbulence. Actually, the choice of laminar input, while understandable in the framework of this work, still represents a significant limitation of this work. I may be mistaken, but I believe that higher turbulence not only could reduce the overall impact of motion on the wake but could also alter the wake response to changes in reduced frequency and motion characteristics more in general. The authors should clarify this point.
- See response to your first comment.

Line 99: I am not sure that the wake deficit induced by the nacelle falls at the center of the wake for all operational cases. Additionally, do the authors have any explanation for why the near wake at x=1D is not symmetric?
- We agree on this. There are some cable bundles that may have caused the asymmetric wake. These cable bundles are located at the downstream end of the nacelle, which is located at ~0.5D. Therefore, the comparatively strong disturbances at 1D may be caused by the vicinity of the cables to the measurement point. It is also possible that parts of the tower wake are deflected into the horizontal plane due to wake rotation. However, we have found no hint that disturbance impacts the mid and outer wake parts, which dominate the impact on downstream turbines. Such behaviour can unfortunately not be completely avoided in our setup, but can also be observed in other studies (e.g. https://doi.org/10.5194/wes-10-1369-2025 or https://doi.org/10.1002/we.502). Again, a deeper discussion on that has to be moved to the follow-up paper.

Line 100: Previous studies have shown that the position of the transition point for the wake is usually affected by upstream turbulence. In particular, no significant differences were observed in the transition position with higher turbulence intensity when motion of the turbine is introduced. This represents one of the conclusions of this work which, in my opinion, cannot be easily generalized to a turbulent inflow case.
- Thanks for this comment. Although, we included the turbulent cases, we do not show plots on this transition. We totally agree with your point that this finding cannot be transferred to turbulent cases without a proper reasoning. We adjusted our formulations regarding the transferability, since -as you mentioned - they have been to general.

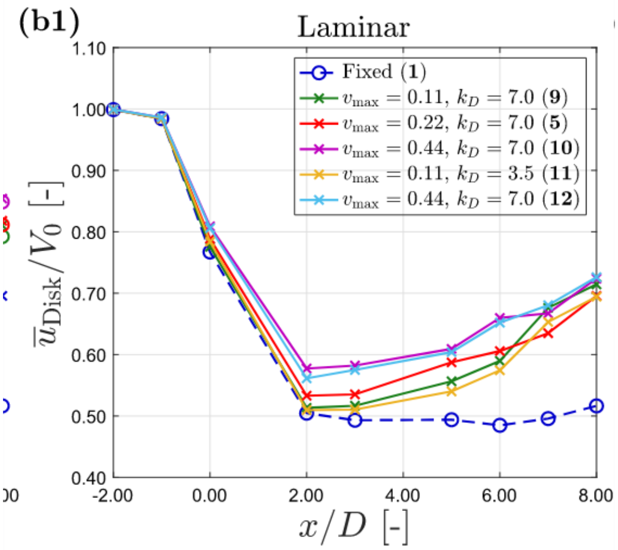
Line 103: the authors state that the wake “returns to fixed-bottom performance” for St= 5.75. However, there are significant differences between the fixed-bottom and motion case in terms of velocity profile. I believe this sentence is due to the velocity profile returning (slightly) to a top-hat shape; however the difference from fixed-bottom case should still be clarified.
- Thanks for this comment. The mentioned expression related to the 8D case. It seems that this was not clear. In addition, the phrase ‘returns to … performance’ is found not to be precise. We changed the sentence accordingly:
By x = 8D, all wakes adopt Gaussian profiles typical of the far-wake, with most dynamic cases (Stp > 0) showing a profile with a smaller velocity deficit than the fixed case (Stp = 0). Comparing the

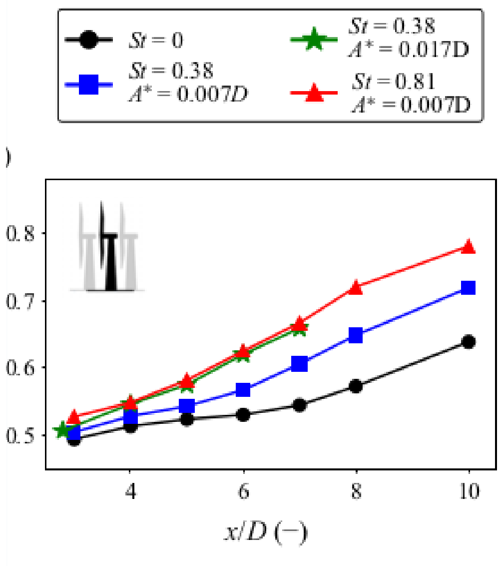
profiles at this downstream position for St = 5.01, 5.39 and 5.75, one surprisingly finds a strong sensitivity on the motion frequency: Stp = 5.39 demonstrates the strongest recovery, while profile at Stp = 5.75 (yellow dotted line) is very similar to the fixed-case again.

Line 105: “after Eq (1)”. Please rephrase.
- Thanks for the hint. We have deleted this reference to improve readability.

Figure 1, b.1 and b.2 the authors should consider changing the colours of the lines to have a match between the two figures at a Strouhal number of 0.6. In particular the similarity between colours for St=0.3 on the left side and 0.6 on the right side can cause a lot of confusion at first glance.
- We agree on this and applied the proposed change.

Line 114 and Line Line 117. I believe that the manuscript may overstate the impact of the induced velocity on the wake deficit. In fact, it is true that the current results confirm the previous study by Garcia et al., where increases in recovery where observed, but in that work, such differences were quite small (ignoring for a moment the limitations of the numerical methodology employed in that work when assessing the wake response up to 20D downstream).
Regardless, also in the current study the impact of the induced velocity on the wake deficit is limited at high Strouhal numbers, even though larger differences are observed for lower frequencies. For this reason, I wonder if this trend should be highlighted in the discussion of the results.
- We agree that the results found by Ramos-Garcia et al. and the methodology may not be very suitable or would need to be explained in a bit more detail to be considered as supporting literature. We therefore removed this reference. However, we believe that the findings of Li (2024) (left figure below) and Messmer (2024) (right figure below) support this hypothesis rather clearly: In the left figure, the green, red and purple line s show the same surge motion frequency but with 2 and 4 times larger amplitude. Here, a clearly pronounced wake recovery with increasing amplitude is visible. In the right figure, the red line shows the wake recovery for the same frequency as indicated by the red line, but with approx. 2.5 times higher amplitude, resulting in a significantly higher wake recover y. Both observations are in line with our findings for St < 1.





We agree on the reviewer's observation that the impact of \Delta V^* (i.e. the amplitude at a constant motion frequency) seems to be less important when it comes to the newly observed phenomenon. However, the results are very dependent on the exact motion frequency while frequency region of interest is not covered with a high resolution. In the newly added cases with added inflow turbulence, we observe a stronger impact.

We added the following sentences to the discussion to make this more clear:

Generally, it appears that the impact of ΔV^* is comparatively strong when $St_p < 1$, while it is less pronounced in the frequency region where the novel effect occurs in the laminar case. However, in the turbulent cases the impact is clearly visible in the complete frequency band.

Figure 2b. If possible I would recommend including the fixed-bottom wake deficit as a horizontal line to highlight any increases in wake recovery.
- Thank you for this comment. In fact, at x = 4D (fig1 a.2) some differences between the fixed case and the surge motion with St = 5.75 are observed in the profiles. It seems that the high frequency motion slightly accelerates the development of the wake at this position. Farther downstream however at x = 8D (fig 1 a.3) the two profiles are almost matching. Accordingly, we modified the text in order to avoid confusion.

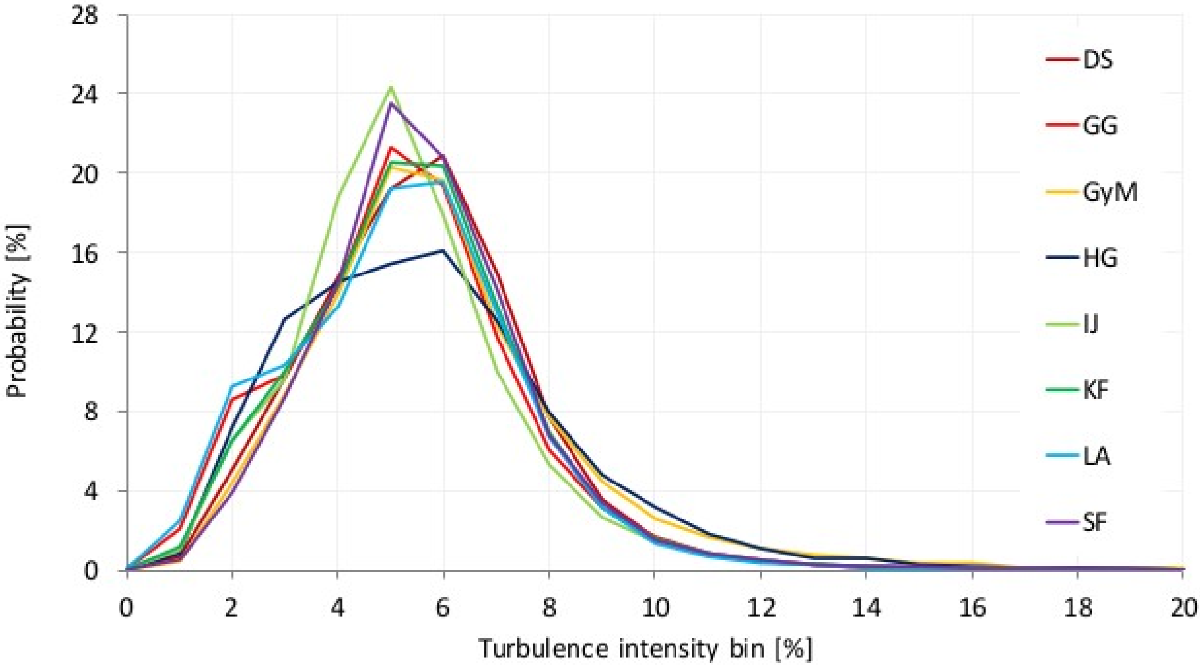
Line 132: I wonder if there were some issues in the final draft of the manuscript concerning the writing of this sentence "[...] whereas the wake in the fixed case (Stp = 0) exhibits broadband energy distribution over $f D/U_\infty \in [0.2, 0.6]$, characteristic of natural wake meandering (Figure 2b)". In fact, as shown in the figure and as I would expect, the wake oscillations in the shear layer should have a broadband spectrum in the fixed-bottom case and a significant response at the frequency of motion for the motion cases. Please clarify or correct this section of the manuscript.
- We agree that the sentence was not clear and proposed a new version. However, we are not sure if this is fully addresses the concerns of the reviewer. We would appreciate a more detailed comment, if needed.

The power spectra of the streamwise velocity fluctuations at $St_p=0.3$ show a dominant response at the platform motion frequency (Figure \ref{fig:recovery_10d}(c.1)). In contrast to this, the power spectrum in the fixed case ($St_p=0$) shows a broadband energy distribution in the normalised frequency region between[0.2,0.6], which is characteristic for natural wake meandering (Figure \ref{fig:recovery_10d}b).

Line 132: If I am not mistaken, the fact that the velocity spectrum is obtained in the shear layer of the wake is reported only in the caption of Figure 2. I think it might improve clarity for the reader if this detail is also reported in the main text.
- Thanks for this hint. We modified the sentence accordingly:
The power spectra of the streamwise velocity fluctuations obtained in the shear layer of the wake show a dominant response at the platform motion frequency when $St_p=0.3$.

Line 174: The authors state that it is interesting how surge motion and rotor rotational frequencies interact at large distances from the rotor. While I agree that this outcome may be in some way unexpected and definitely interesting (especially in the comparison drawn to the helix method), I believe that the current results are strongly a ffected by the use of a laminar inflow during the tests. In fact, under laminar inflow, the vortex structures in the wake are much stronger and more persistent (as the turbulent fluctuations alter the stable location of the tip vortices, promoting vortex interaction and collapse, hence aiding mixing), and periodic harmonic perturbations as induced by a sinusoidal surge motion may have very interesting e ffects on wake recovery. For this reason, it could be possible that these coherent structures and wake dynamics are practically not observable under realistic inflow conditions (and probably already for low turbulence levels).

- We understand the reticence of the reviewer about this phenomenon. As suggested by the reviewer, we added a preview on the results in turbulent inflow condition, which show that the impact of the novel phenomenon persists for most turbulent cases and its strength is comparable to the effect occurring in the low frequency region. The results indicate that an impact of the novel phenomenon persists up to 4.5% TI in the considered surge motion case. Situations with a TI below this threshold actually occur frequently at different locations in the north sea (see Figure below). However, wind turbulence generated in wind tunnels is not directly comparable to real conditions, which is the reason why we do not draw this conclusion in the paper. Nevertheless, this may be considered as a first hint that it might indeed be possible to observe the new phenomenon in reality.



Turbulence frequency distributions for 8 – 10 m/s wind speeds

P. Marek et al., A study of the variation in offshore turbulence intensity around the British Isles, Poster, Wind Europe Summit 2016, https://windeurope.org/summit2016/conference/allposters/PO293.pdf

As physically motivated researchers, we are not only interested in phenomena that have an immediate impact on the results in full-scale, but we try to understand the underlying phenomena. Since we are not aware that this kind of phenomenon has been claimed or observed in the context of wind turbines or other rotors, it is likely that the observations we made in this experiment actually describe a previously unknown physical phenomenon. If this is the case, we believe that the discovery of this effect has its own, significant scientific value - even without its relation to wind energy. Nevertheless, this manuscript is just the first hint on such phenomenon.

Line 232: The authors state that the trends deduced by this study are persistent over increased turbulence levels up to 5%. However, unless I have not missed something substantial in the manuscript, no experimental data is provided in the manuscript to support this statement. As explained in the general comment, the authors should further expand the discussion on this point.

- See our answer to your general comment.

2 Reviewer No.

Manuscript titled “A novel wake mixing phenomenon and key parameters for wake recovery of floating wind turbines subjected to surge motions” discusses the impact of motion conditions and turbine operating conditions on the wake deficit and coherent structures. This work is well structured and presented and presents a valuable addition to the current understanding of wake dynamics of floating wind turbines.

Still, there are some additional comments that should be addressed:

The authors are only presenting results under laminar inflow conditions. Having that in mind, without data to support it, or more literature that would support the claim, the extension of the presented results to turbulent inflow conditions is way too strong statement and should be revisited. In the light of some existing literature on floating wind turbine wakes, implying that turbulence may have a significant impact on wake dynamics, author’s claim(s) should be supported with data or reframed. Also, the presented (included) body of literature on impact of motion on the wake of a floating turbine should be a bit expanded.
- We are grateful for this objection . After some time has passed and we reviewed the manuscript ourselves again, we clearly see the point of the reviewer and are very happy to receive this feedback. We decided to include the results under turbulent inflow, which has unfortunately caused some delay of the revision.

Additional comments:

Line 25: The authors state that the unsteady impact on the loads can be determined as a function of the motion frequency or Strouhal number. I believe that reasoning of using these specific parameters should be included.
- After reviewing the mentioned sentence again, we see that it was too complex and misleading. We changed it to the following version:

From these studies, the main results were that the unsteady impact on the loads caused by harmonic tower top surge motions can be characterised by two parameters, the rotor reduced frequency i.e. platform Strouhal number, and the relative rotor velocity $\Delta V*$, which is the ratio of the surge velocity amplitude and the incoming wind speed. F

We hope that we could address the reviewers concerns in this way. If not, please let us know.

Line 69: here “Nevertheless, the results can be generalized to turbulent uniform inflows, however, not shown here.” should be better explained (either discuss data that supports this statement, or study that will support it). So, unless the authors include better proof that laminar flow experiment measurements/results are applicable to higher turbulent flow, this should be excluded/revisited.
- See our statement above. We changed the sentence to

In addition, reduced measurement series utilising an active turbulence grid are performed to demonstrate that the laminar results can be generalised to turbulent uniform inflows up to a certain level of turbulence.